Peer Review Comments - Technical Issues **Comment**	**Response**
Comments of Ivar Tombach Concerning Both Documents:	
In the Glossary of Terms, the definition for *Crustal Material* as the sum of the soil mass and the coarse mass is not correct. The soil mass that is mentioned must be that of the *fine* soil. As for the coarse mass, it has been found to include not only crustal material, but also organic material (such as plant detritus), sea salt, and sodium nitrate (a reaction product of sea salt and ammonium nitrate, as mentioned in TP Section 3.3). In some studies only about half of the coarse mass has been found to be crustal. Thus the definition provided is not sound. I wonder, however, why the definition is needed at all – I don't recall seeing the term used anywhere in either document.	The definition has been removed.
In both Introductions, there is mention that the document may be revised periodically without public notice (third line from end of NC Sec 1.3 and TP Sec. 1.2). This statement makes me uncomfortable. I'm sure that the States and the regulated parties need to know that the rules of the game will not change capriciously and that some sort of due process will occur when changes are made.	EPA reserves the right to make minor changes to the documents without public notice. Public notice will be made in the case of Substantial changes. No change needed.
The documents foster considerable confusion about the deciview. The deciview is a unit of measurement of haze; the quantity being measured is a haze index (here denoted by HI), which is defined by the formula $HI = 10 \ln(b_{ext}/10)$, where the units of HI are dv and the units of b_{ext} are Mm^{-1}. The formulas in various places in the two reports use the term deciview to describe both the quantity being measured (HI) and the units in which it is being measured (dv). Further, in Section 1.6 of both NC and TP, there is the statement, "The deciview is a haze index derived from calculated light extinction…." This is wrong. Rather, the haze index that is derived from the light extinction coefficient (either calculated or measured) is measured in deciviews. The title to TP Section 3 and the caption to TP Figure 3-1 have the terminology correct.	Documents have been revised to clearly distinguish the haze index from the dv units.

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The table that provides the specific values of the "official" f(RH) function (Table A-1 in both documents) is a good idea, although giving the values to four decimal places is absurd. I sure would like to know where that function, which differs from the one that has been used by IMPROVE for many years, came from, though.	Table A-1 values have been limited to two decimal places.
I suggest that something other than 0.00 be used to denote missing data for the Bering Sea Wilderness and Virgin Islands NP entries in Appendices A and B of both documents. The value of 0.00 is a legitimate value and could be used unwittingly in a computer program. Either the field should remain blank or an absurd value such as –999.99 should be used.	The fields have been left blank in the revised documents.
Ivar Tombach Comments Concerning the Natural Conditions Document:	
NC Section 1.7, footnote 7 is redundant. The same statement was just made in Section 1.6.	Footnote 7 has been removed.
NC Section 2.6, footnote 14 needs to provide information as to where the Ames and Malm paper cited there was published or how it can be obtained by the reader. Also this paper is cited as both references 14 and 10 in the same paragraph in the text.	Footnote has been revised and numbering has been changed.
NC Section 2.6. The presentations in Figure 2-1 and 2-2 make me uncomfortable. In Figure 2-1, there is about a 0.5 dv step across the gap between the two halves of the country, but that discontinuity happens to be obscured by the relatively good color match across the gap. In Figure 2-2, the same colors appear in eastern Texas and in Louisiana, which suggests similar haze index values, whereas in reality there is a 3 dv step across the gap. These steps can be operationally troublesome for the regional planning and modeling, and their existence should be acknowledged more openly. At the very least, the figure captions should point out the difference in scales (in addition to the comment in the text). It might also be appropriate to point the reader to the new comment in the second paragraph of Section 2.1 that gives States near the boundary discretion in selecting which values are most appropriate.	Changes have been made in the figure captions.
NC Section 3.3, next to last sentence is confusing. It took me several readings to figure it out. I think it is trying to say, "Such refined approaches may require alternative methods to predict the 90^{th} percentile of natural conditions."	Sentence has been revised.

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NC Section 3.4, second sentence. The comment in parentheses could be misinterpreted (as I did) because it does not make it clear that "any time period" means the identical period in time for both the natural conditions and the measurement. A wording like, "i.e., in any given time period, the natural particle species concentration cannot exceed the measured concentration," would be better. A similar sentence, with the same ambiguity, appears in the second paragraph of Section 3.6. It could be improved by a similar rewording.	Sentences have been revised both in sections 3.4 and 3.6.
NC Section 3.5. The process here also took a bit of divining to figure out. I think the third sentence is trying to say that one can look at the change in concentrations that was measured during a present-day fire and assume that the same increase in concentrations would occur if the fire had taken place under natural conditions.	Revision was made.
Ivar Tombach Comments Concerning the Tracking Progress Document	
TP Section 1.8, fourth line after Figure 1-1 should read,"… this rate of improvement would lead to a reduction in the 20% worst average value *by* [not *of*] 4.2 deciviews."	Revision has been made.
TP Sections 1.11 and 4.4 discuss the every 5 years progress review. I will be presenting a paper at the A&WMA Annual Meeting in Baltimore in June 2002 in which I demonstrate that it is likely that there will be ambiguity in determining whether the haze index on the 20% clearest days is following the required behavior of remaining constant or decreasing. This ambiguity occurs because of uncertainties in the measurement of the chemical components of fine particulate matter. The contribution of measurement uncertainty to ambiguity in the determination of trends in the haze index needs to be recognized in the guidance. Perhaps tolerances of acceptable trends need to be allowed. For example, if the requirement is to prevent degradation on the 20% clearest days, an *increase* or *decrease* of 0.1 dv in any single 5-year period should be considered equivalent to no change (with 95% confidence), based on my estimates of measurement uncertainty. (This isn't an issue on the 20% haziest days because the required "glide path" slopes are steep enough, even in the West, that the ±0.1 dv measurement uncertainty is not great enough to make the direction of the actual slope ambiguous as long as the actual trend approximates the required slope. The quantification of the actual slope in the West may be pretty coarse, however.)	It is recognized that there will be uncertainties in determining the trends, especially for the clearest days. EPA has not recommended acceptable tolerances for the trends, but would expect that they may be included in the SIPs. However, it should also be noted that the haze index on the cleanest days should not be allowed to "creep up" over time as a result of these uncertainties.
TP Section 1.11, first sentence of second paragraph. Add the word *action* as the last word of the sentence.	Revision has been made.

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TP Section 1.13, second paragraph, second sentence. The IMPROVE network is now fully deployed, so this paragraph needs updating.	Revision has been made.
TP Section 1.14, last word. Add *sites* or *locations*.	No change needed.
TP Section 2.2, Step 3, first sentence. I can't figure out what this sentence is trying to say.	Sentence has been revised.
TP Section 2.2, Step 5, 4th paragraph. In the last sentence *criteria* should be *criterion*.	Revision has been made.
TP Section 2.2, Step 5, 6th paragraph. The last sentence seems nonsensical and unnecessary. In a trend assessment one would usually look at a trend at a given site. That site remains at a fixed elevation, and so any variation of Rayleigh scattering with elevation is irrelevant.	The offending sentence has been removed.
TP Section 2.2, Step 5, 7th paragraph. Mention is made of elemental carbon and of EC. The rest of the document uses LAC to represent the same quantity.	Revision has been made.
TP Section 2.3, first paragraph. The 4th line mentions 123 sampling periods per year. I think 122 is the maximum possible. In the sentence beginning in the 9th line, the first words should be. "Thus it is reasonable …."	Both revisions have been made.
TP Section 3.2, second sentence. I don't understand the point of this sentence. As I see it, the species concentrations needed for calculating the extinction coefficient and then the haze index *are* measured directly.	The sentence has been revised.
TP Section 3.2, Figure 3-1. Why does the top box require estimates of *Natural* concentrations?	Both figure and caption revised.
TP Section 3.3, first paragraph. The sentence that describes Module A seems to be lacking a few words.	Revisions have been made.
TP Section 3.3, third paragraph. The sentence before the formula should begin, "The dry *ammoniated* sulfate value …."	Revisions have been made to this section.
TP Section 3.3, last paragraph. The last sentence is grammatically challenged. More importantly, what is the implication to the progress tracking process if sea salt or sodium nitrate are major constituents of coarse mass?	Revisions have been made to this section.
TP Section 3.4, last paragraph. In the next-to-last sentence, the first word should be *Based*.	Revisions have been made to this section.

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TP Section 3.5, third paragraph. There is a problem with logic here. The second sentence says the sulfate f(RH) function is quite different for the East than the West. Two sentences later, a single curve is used to represent both regions. The reasoning that allows dismissing the "quite different" functions and replacing them with one is missing.	Revisions have been made to this section.
TP Section 3.5, discussion about f(RH) for organics for the next couple of pages. This is an interesting discussion and it provides some basis for the arbitrary assignment of f(RH) = 1 for organics, but it is far too detailed a digression. No other technical issue in the document takes up two pages, and the discussion goes into detail that will elude the average reader of this document. I suggest that some editing would be helpful here.	The discussion has been revised and reduced.
TP Section 3.8, last paragraph, first sentence after the formula for estimating light extinction. I know I am fighting conventional misusage here, but the formula cannot be used to *reconstruct* extinction, but only to *estimate* it. (Reconstructing extinction is a very different process.) The formula is full of assumptions and approximations that make it possible to calculate an estimate; one can change those assumptions to get a different estimate. In the critical review that John Watson is preparing for the A&WMA Annual Meeting in June, he uses *chemical extinction* to describe the quantity represented by that formula (and others like it). I like that term. I don't recall that the word "reconstruct" is used anywhere else in either document than in this paragraph and the next one. I suggest that it not be used here, either	Revisions have been made to use "calculated" rather than "reconstructed".
TP Section 3.8, last paragraph, last sentence. A personal communication is like hearsay; the reader has no way to investigate how the information that was communicated was derived. Either this last sentence should be deleted, or it should be expanded enough to explain the basis for the claim. In particular does the 1.2% represent relative error or relative absolute error? If the former, it says very little about how large the differences were from day to day because large errors of opposite sign can cancel out to give a small average.	Revision has been made to this section.
TP Section 3.9, first sentence. The first sentence both uses the word "reconstruct" (see my comment above) and is confused about deciviews. In my eyes, a better wording would be, "Once the light extinction has been calculated for a monitoring site, using Equation 8, the haze index expressed in deciviews can be calculated." (Incidentally, I'm using "Equation 8" above because that was the number used in the draft; equation numbers got lost in the conversion of your document into something that I could read. My counting of equations suggests that it might actually be Equation 9, however.)	Revisions have been made, including indicating the correct equation numbering.

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TP Section 3.9. Most of this section largely repeats Section 1.6, but gets some of it wrong – see my earlier comments about the haze index and deciviews. It could be trimmed a bit. Also, there is enough material in the recent scientific literature to put into doubt the claim that a one dv change is just perceptible under a wide range of conditions. Rather than get drawn into that controversy, I suggest just deleting that portion of the sentence; it is not needed for the purposes of this document. Finally, the final sentence of this section should begin, "The value of the haze index is near zero dv for a pristine atmosphere …."	Revisions have been made.
TP Section 3.10. The process of choosing the clearest 20% and haziest 20% of the days is essentially one of selecting two groups of "outliers" in a distribution. Actual outliers will have a greater effect on these tails of the distribution than on the 5-year averages discussed in this paragraph. Thus, I believe the percentages given here understate the effect of excluding outliers in the data for the clearest or haziest 20%. In fact, the values selected to define either 20% group will change depending on whether outliers are included or excluded.	Changes have been made to clarify that the calculation was performed on the tails and not on the overall distribution.
Comments of Rich Poirot on Tracking Progress Document	
In Glossary: Generally several of these definitions differ from those for same terms in the Natural Background document. Seems like they should be made the same. The definitions in TrakProg generally seem more careless, for example: 1. Crustal material 2. Fine soil 3. Nitrate 4. Organic carbon	The glossaries have been revised and made consistent.
Section 1 P. 1-7 "State ... is required to consult with **nearby** states having Class I areas that may be impacted..." I think "nearby" is vague, and could/should be deleted or defined more specifically.	"Nearby" has been replaced with "other".

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P. 1-10 "the energy and **non-air quality environmental impacts** of compliance" While this phrase, directly from the statute, has never been clearly defined, perhaps there would be some opportunity to define "impacts" broadly (to include "benefits") in this guidance, by example. For example: "Non-air quality environmental impacts might include effects on aqautic, terrestrial or materials damage from acidic deposition, eutrophication of coastal estuaries from nitrogen deposition, changes in the deposition of toxic trace metals or organics that may result from emissions changes, or changes in visibility or 'air quality related environmental values' that might result in non-Class I areas".	Revision has been made.
PP 2.2&2.4 The fine soil reconstructed mass procedure calls for substituting MDL/2 if any crustal element is <MDL. The IMPROVE data (historically, and I'm not aware that the problem has been corrected) exhibit a problem with the quantified MDL for Al. Specifically the (laboratory-calculated) MDL for Al is too low. ...This problem is "correctable" - or at least could be improved upon by something like: "If Al is below MDL, rather than substituting MDL/2, it is recommended to leave Al *(Cont'd)* out of the soil calculation and to increase the Si factor from 2.49 to 3.73. This recommendation is based on what appears to be a mis-quantified MDL for Al in the historical IMPROVE data. The 50% increase in the Si factor is based on an observed, consistent relationship at most sites of approximately Al = Si/2, when only above detect Al and Si concentrations are compared. Possibly this Al:Si ratio can be improved upon or refined on a site-specific basis. If both Si and Al (the 2 largest soil components) are below detect, it is likely that other soil elements may also be below detect, and that fine soil concentrations are very low. For these samples, it is recommended to substitute each specie's MDL/2 in the soil calculation".	We agree, however, the procedure presented summarizes the current practice used by IMPROVE. Unless changes are adopted by IMPROVE, these procedures will be used.

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PP. 2-4-2.8 **Assess Missing Variables**. Clearly this recommended "hole-filling" procedure has resulted from a lot of careful thought and group discussion. So I'm sorry to have to say that I think it is entirely too complicated, not particularly well justified and not clearly explained. I have a hard time even following the recommended procedure, and whenever I think I'm understanding what's being proposed, I get stumped because I can't follow the "logic" or objective of the approach. The level of effort & detail required for these calculations does not seem justified by an associated improvement in the accuracy or reduction in uncertainty of the resultant regulatory metrics. At a minimum, there is a need for a clear, up-front discussion which describes why the missing data substitution issue is important, and also explains what the proposed procedure is intended to achieve. The Q&A format, used effectively elsewhere, could be useful here - for example "Why are the missing data procedures so complicated?" Other comments on this missing data process follow below:	The rationale and practice for hole-filling has been revised to better explain the need and the method to meet the need. Additionally, the calculations described in this section are performed by IMPROVE.
1. Frequently, parallels are drawn between these requirements and those for other federal monitoring/compliance programs. However a key difference here is that (with few exceptions) the haze monitoring responsibility has not been delegated to the States/Tribes. The States don't operate the sites, and have no chance to "game" the monitoring (for example the old 'shut off the sampler during episodes' trick). Nor do the states have any opportunity to improve upon future data capture efficiency at sites they don't operate. They are however delegated the responsibility ("penalty" would be an appropriate term) of making a lot of tedious calculations in the event that the Federal monitoring program screws up at their site(s). If these calculations will be routinely and automatically provided by the IMPROVE program, this should be clearly stated up front, followed by an indication that these are the procedures that will be employed, which are stated below in the event that States may wish to reproduce them and/or propose alternative procedures which may be negotiated with EPA on a site by site basis.	The calculations for data substitution are performed by IMPROVE such that the responsibility does not fall with the States.
2. It would help if "Percentiles" were defined clearly - including any rounding or truncation procedures. Are they statistically projected, the data values closest to the 20th and 80th percentiles, or the data values just below or just above the 20th & 80th percentiles? "Quarter" is not defined (that I can find) and if not it needs to be.	Revisions have been made to the document to address these two issues.

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3. Assuming it's "calendar quarter", I don't follow the logic of excluding a previous year's quarter if its missing a contiguous month. What if the missing data point occurred in January, but the preceding year was missing March? Arguably, the preceding Jan & Feb data are more likely similar to this year's Jan data than last years (missing) March data would have been. There's a logic flaw in first assuming that all months in a quarter are equally valid estimators for estimating a value in any of the months, and then excluding all of a quarter's data if it is missing any 1 month. The calendar quarter averaging almost certainly "skews" the averages away from representing the 1st & last months in the quarter (ie April is nearly always cleaner than June, and at high elevation sites, Dec and Jan are usually cleaner than Oct and Mar). A more logical approach might be to define the 3-month averaging period as a "floating" one, centered on the month with missing data point, in which case the missing month criteria could be dropped.	Though the reviewer may be correct that the method can be improved on, he has just suggested an even more complex approach after having complained above about the current approach being to complex. No revisions made to process or document.
4. While there are strict data completeness requirements for eliminating preceding years from a calculation of 5-year quarterly means, I don't see any restrictions on the minimum number of "valid" years (quarters) needed to calculate a "valid" 5-year quarterly mean. The limitation of 'at least 3 of 5 years' is not employed until later in the 5-current year block average calculation process. So in theory, a missing data point this June could be filled by substituting a "5-year quarterly average" determined from as few as 15 data points, including only 1 in June, 1 in May and 13 in April from 5 years earlier. Considering that the regulations require a (decreasing) time trend, use of the 5 preceding years would introduce a high bias to the estimated averages for improving species. If the required progress is 100% of the difference between baseline and background in 65 years, the trend would represent a change of about 1%/year - which for a preceding 5-year block would introduce a positive bias of about 3%. The bias will be greater than that for controlled species if some species are reduced and others are not, and/or if the most recent years fail the completeness requirement. At a minimum, I don't see why the current quarter (the one in which the missing data point occurs) should not be included if it meets the completeness criteria.	The reviewer has a point about including the current quarter in the calculation of quarterly values for substitution. Revisions to this section have been made to include the current quarter.

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5. Use of preceding 5-year averages has the (slight) "advantage" that they can be pre-calculated in advance of a new quarter's data, so the tests and (if passed) the hole filling can be done instantly and automatically - providing quick "certainty" to the haze-relevant metrics. However, this advantage is very minor, since it only provides "useful" information once a year when the calendar year's data are complete. And even this is only marginally useful, as the critical determination is only possible at the end of each 5-year block. A question is: will this elaborate, biased hole-filling procedure really provide critically useful information to the states (compared for example to the alternative of estimating how they're doing on the basis of non-missing data only)? If the goal is to provide the best possible estimates for missing species, then use of averages derived from the "current" 5-year (5-quarter) data block would (on average) not be subject to the trend bias that will characterize the preceding 5-year block.	See comments above and below.
6. (sorry to suggest this, as I bet the work's already been done but) I really think the median would be a better estimator of missing data than the mean. For most of the (episodically distributed) visibility-relevant species, the mean is higher (about 1/3 to 2/3 higher) than the median, and is likely closer to the 60^{th} or 70^{th} percentile. Substituting the mean introduces a (second form of) high bias into the estimator, and is therefor likely to shift clean days into the middle bin and middle days into the high bin. Its also more likely to result in a failure of the substitution test - for no good reason.	Revisions to the document have been made to indicate that medians will be used in the calculations not means.
7. P. 2.7 The procedures for making these data tests are not clearly stated, but seem to imply that for any missing species, the extinction with substituted averages test needs to be compared to actual extinction on an annual basis. I assume this is not really what's intended, and that - for example if some nitrate values were missing during a summer month, the test would be applied to only the summer quarter. Whether average nitrate would pass or fail the test for other quarters (during winter, for example when nitrate is always higher and more variable) is irrelevant. Also am not sure where the number "35" (potential tests) comes from. There are 6 species and 5 years (or, I think 5 years of each of 4 quarters). Thus there could be a potential of up to 20 tests for each of 6 species = 120 tests = absurd...	The substitution procedure calls for substitution of appropriate quarterly values (i.e., data from a summer month should be replaced with the average of the median values for the summer quarter during that 5-year period). The number of tests has been changed to 30.

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8. The "test" (demonstration that substituting quarterly mean would not change daily bext by > 10% on more than 90% of days) seems a bit arbitrary, and doesn't seem directly relevant to the data questions of concern - which I think are: a. 5-year mean of the 80th percentile reconstructed extinction, b. 5-year mean of the 20th percentile reconstructed extinction, c. Fractional contribution, by species, to 5-year mean of the 80th percentile reconstructed extinction, d. Fractional contribution, by species, to 5-year mean of the 20th percentile reconstructed extinction, The first 2 metrics are the key to tracking progress, while the latter 2 metrics are important for development of and evaluation of control strategies; the last thing we want is a misguided control strategy, or an inaccurate measure of its effectiveness. "Stability" of the regulatory metric is also desirable. All else being equal, we don't want a 5-year average composed of widely varying annual metrics. I think then, that whatever hole filling and test procedures are employed, consideration should be given to potential effects on all of relative to these metrics above. The missing data substitution might be allowed (required, or recommended?) if it does not result in more than an x% change in either a or b, nor more than a y% change (for any individual species) in either c or d (as calculated using only complete years with non-missing quarters. But this gets silly, since then the missing data can only be filled if they make no difference - in which case what's the point?	This section has been revised to better explain this test.

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9. P. 2-10. There's a reference to an "obviously bad (or clean) year", with procedures to assure they get included. I question the concept of "obviously bad year", first because there's no logical emissions modulation or meteorological anomaly that extends specifically to a 1-year period. Fires in the spring do not predict more fires in the fall; winter stagnation does not predict summer sulfate, etc. Second, if there were such a thing as a "bad year", I don't think it can be judged to be "obviously so" on the basis of just the 33% of days that are sampled (presumably more like 25% of total days, since this procedure is only advocated for years that fail the completeness criteria). Still, I think there is substantial uncertainty in knowing whether the "bad" refers to anomalously bad pollution or to bad luck in which a high fraction of valid sample days happened to coincide with episode days. And then, if the year was in fact an anomalously bad pollution year, how badly do we really want to include it in this tracking progress metric anyway? If it occurs in the baseline, it creates "artificial" success at the next checkpoint, or artificial failure if it comes in the 2nd 5-year block. In any event, I'm not sure the proposed procedures do bring a calculation "closer to its true value" (and the "true value" of a 5-year mean of percentiles is a pretty illusive concept in the first place). In addition, since this exception only gets invoked for years which tend to depart from others in the 5-year block, it would tend to decrease the stability of the regulatory metric, which is not desirable (an incomplete year can only be included if its anomalously different from others in its group). I also dislike the use of (different) incomplete years for clean and dirty purposes separately. Presumably if its truly a dirty year, its "true" 20th percentile values might also be expected to be high relative to other years in the block.	It's not that this is a year that is an outlier very bad year, it is an issue of a year that is missing data during periods that don't contribute to the best or worst conditions so that the available data are sufficient to contribute to the trend. The way that has been selected to make that determination is by comparing the worst days with available monitoring data to the worst days in other years of the five year period. The text has been revised to clarify this concept.
10. A direct analog to an "obviously dirty year" (and one which is less ambiguously obvious) is an "obviously dirty day" which might have a missing species. If the partially reconstructed extinction for that day using only non-missing species would (if added) exceed the 80th percentile for that year, then including its partial extinction would bring the years 80th percentile "closer to its true value". Substituting the average (or median) for that day's missing species would not affect whether it belongs in the high bin, and would likely have a minimal effect on the average fractional extinction (for high days) for that missing species, but might improve the estimate of fractional extinction for the "obviously dirty species, and so it would be desirable to allow that substitution	Same as above.

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11. I don't see good reasons to assume much bias (high or low) to the missing species on the days when they are missing. I can't think of any reason that a clean day would tend to have missing species; if they are below detect, they're obviously low and MDL/2 is used. I wouldn't expect sulfate (redundantly measured as either 3S or SO4) to ever be missing (or if it was, we would also be missing everything but OC & EC and can't hope for much of a reconstruction). Possibly all filters will clog during an extreme local fire event, but then there's no hope of reconstruction and some logic to exclusion as an "exceptional event". Otherwise, there's no good reason for EC or OC to be missing on high or low carbon days. Probably coarse mass (requiring 2 successful weighings of 2 different filters) is typically the most frequent missing species, however, there's no good reason for it to be missing on high (or low) coarse mass days. The only possible bias I see is that nitrate (& SO4) may be more frequently missing on high (sulfate or organics) mass-loading days due to unacceptable flow rate drops on the nylon filters, but this would not necessarily affect the S or OC data, and nitrate is not likely to be especially high (or low) on high sulfate or fire days. I would be tempted to do away with this whole missing data testing procedure, and allow it only in the case of missing nitrate (the only likely "biased miss"), and only in the event that substituting the 5-year quarterly average nitrate for that sample did not increase that day's reconstructed extinction by more than 10% over the reconstructed extinction if nitrate was set to zero.	This section was rewritten to better justify the missing data procedures.

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Section 3 P. 3-4 I don't think the equation 1 (alternative sulfate calculation if ammonium is available) is helpful (or accurate) and might be deleted. First, there are currently attempted NH4 measurements at only a few sites, and these are likely "upper bound" values, since no precautions are taken to prevent reactions with gaseous NH3 during sampling, or subsequent filter handling. There is also an indication that (Module B) extraction procedures to allow NH4 analysis have effected the extraction efficiency for (the more important objective) nitrate - and so quite possibly this experimental procedure may be discontinued. It is not a valid assumption that nitrate on the nylon filter is present there as ammonium nitrate. Its associated ammonium may be lost, even as the nitrate is retained by chemical reaction on the filter, and/or a significant fraction of the nitrate may be in the form of sodium nitrate. As proposed, it looks like this procedure would also use the SO4 ion data, rather than the S data (otherwise preferred and likely to indicate a higher level of sulfate in recent years than SO4 ion data does). Thus both the SO4 and NO3 data are likely biased low where this (dubious) procedure is recommended. It is also not appropriate to combine an (inaccurately estimated) acidic sulfate species with the hygroscopic grow curve for ammonium sulfate. Water affinity increases with increasing acidity, and a set of alternative growth curves should be developed (but are not mentioned in the guidance) for more acidic compounds. At higher acidity (& RH) levels, the increased scattering from water will exceed the mass (& reduced dry scattering) lost from the reduced mass calculation for the sulfate compound.	This section has been revised to clarify these issues.
Comments from John Watson on Both Documents	
1. The f(RH) growth curve needs to be documented and justified and made consistent across all the (related guidance) documents. The one used for the two (regional haze) guidance documents appears to differ from that used in the FLAG and attainment guidance documents. The rationale in Bill Malms' "secret memo" should be included in the tracking guidance.	Revisions have been made.
2. Using the RH>95% in the 24-hour averages is not justified by the precision of the measurements used. Bill Malm notes this as well in his memo. Extinction is very sensitive to RH in this region.	Agreed. In calculating the tabulated f(RH) values, if RH exceeded 95%, f(RH) was set equal to the f(RH) for 95%.

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3. There needs to be more justification for the default extinction efficiencies selected and a discussion of their uncertainties. It's not clear from the guidance that the purpose is to approximately, but consistently, weight the effect of different chemical components on extinction, not necessarily to replicate what would be measured. The attached (Critical Review) document discusses this in greater detail.	Agreed, the text has been revised to address this.
4. The 1.4 OC multiplier is not scientifically justified. On the other hand, it does seem to account for most of the reconstructed mass. See critical review discussion on this.	Revisions have been made
5. The NAPAP natural background estimates should be better explained with statements about where they came from and their uncertainties. It would be good to re-examine recent literature that updates that of Trijonis et al., (1990) 15 years ago. I took a crack at it in the attached (Critical Review) document, but I ran out of time to do it more thoroughly. I think this merits more effort than has been invested.	It is agreed that updates to the Trijonis values would be valuable, however, until updated values become available the 1990 NAPAP values will be used. No change to the document has been made.
6. There is substantial evidence that natural conditions (fires, dust storms, biogenics) can cause some of the poorest visibility conditions. These can and should be identified, as noted in the attached (Critical Review) document.	This statement is not inconsistent with anything in the guidance. No change.
7. Deciview trend tracking obscures trends in chemical contributions to extinction. These trends also need to be tracked. Natural fires can obscure the detection of improvements unless identified and dealt with.	Agreed. Section 4.7 of the Tracking progress document discusses tracking trends in the individual species, and it's importance has been emphasized.
8. Natural background and non-U.S. contributions really can't be separated by this regulation. Sulfate, carbon, and dust data from Denali and Mauna Loa are distant from continental emitters and might provide better estimates for "global" contributions from these sources. Establishing other global-scale sites should be considered as part of tracking natural conditions.	Interesting suggestion, but probably is impractical. No change to the document has been made.
Comments of Warren White on the Natural Conditions Document	
I can't find any footnotes or endnotes or references to go with the many superscript numbers in the text.	Footnotes are present in the document; no change needed.

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Section 1.14 claims "Increases in prescribed fire are expected to result in a reduction in wildfire emissions over time." Exactly who expects that? I've seen no forecast that the increased-burn regime will yield total fire emissions in 2064 that are actually lower than those during the fire-suppression regime that the 1990 NAPAP estimates reflect.	This sentence has been removed.
Section 1.14 further claims the 1990 NAPAP estimates for natural background should already reflect "the natural regional contribution by fire", because they were based on studies "conducted in relatively remote areas". Does the Agency really think that remote fires were left to burn naturally back then, and not suppressed. I think most natural historians of fire in America would figure that emissions throughout the U.S. were suppressed substantially below natural levels during the period reflected in the 1990 NAPAP estimates.	The text has been revised.
In Table 2-1, footnote c is wrong: Trijonis' estimates were 1.5 :g/m3 and 0.5 :g/m3 of organics (i.e., organic compounds), not organic carbon. The basis for changing them is thus unclear.	Footnote c has been revised.
The URL http://www.epa.gov/ttn/oarpg/gener.html given in Appendix A doesn't seem to list the promised draft report.	The report has been put on the website.
Same comments on the glossary in the Natural Conditions document as made for the tracking Progress Document	See below.
Comments of Warren White on the Tracking Progress Document	
Glossary: **Aerosols** – Suspensions of tiny liquid and/or solid particles in the air. [AEROSOLS ARE NOT THE INDIVIDUAL PARTICLES THEMSELVES.]	Revision has been made.
Glossary: **Elemental carbon (EC)**– Often referred to as soot or light-absorbing carbon (LAC). Ambient elemental carbon measurements represent the carbon that was not converted to carbon dioxide or carbon monoxide during combustion processes. THIS IS NOT WORDED AS A DEFINITION. MORE PROBLEMATICALLY, IT CLASSIFIES THE WHITE FOG FROM AN OIL-BURNING CAR AS EC.	Revision has been made.

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Glossary: **Organic carbon** – The portion of carbon in particulate matter contributed by organic compounds, which may result from emissions from incomplete combustion processes, solvent evaporation followed by atmospheric condensation, or the oxidation of some vegetative emissions. [NOTE THAT OC IS NOT OMC, ACCORDING TO CHAPTER 4.]	Revision has been made.
Glossary: **Particulate matter** – Any substance, except pure water, [SO DOES DIRTY WATER COUNT AS PM? NOTE THAT YOU INCLUDE NITRIC ACID IN THE NITRATE DEFINITION, AND HNO_3 IS NOT GOING TO SHOW UP IN THE PARTICLE PHASE WITHOUT SOME WATER.] that exists as a liquid or solid in the atmosphere under normal [WHAT'S NORMAL?] conditions and has an aerodynamic diameter less than 10 microns (in the discussions of this report). [PROPOSED ALTERNATIVE] *Material that is carried by liquid or solid aerosol particles with aerodynamic diameters less than 10 microns (in the discussions of this report). The term is used for both the in situ atmospheric suspension and the sample collected by filtration or other means.*	Suggested revision has been made
Glossary: **Visibility impairment** – Any humanly perceptible change in atmospheric transparency or coloration resulting from added particulate matter or trace gases. . [IF IMPAIRMENT IS DEFINED IN TERMS OF "PREVIOUS" CLEANER CONDITIONS, THEN YOU'LL HAVE THE BURDEN OF ESTABLISHING THE EXISTENCE OF CLEANER CONDITIONS IN THE PAST, WHICH IN SOME CASES WILL BE HARD TO DO.]	This revision has been made.
Chapter 3, Section 3.1 - [TWO COMMENTS: 1.GIVEN THE GLOSSARY'S CLAIM THAT RAYLEIGH IS 10 Mm^{-1} AT 1.8 KM ELEVATION, THE CLAIM HERE IMPLIES THAT ELEVATIONS "at most locations of interest" ARE ABOVE 1 KM. THIS MAY BE LITERALLY CORRECT IF "most" MEANS ONLY "AT LEAST 50%", BUT IT SEEMS NEEDLESSLY DISMISSIVE OF MANY BEAUTIFUL PLACES. 2.RAYLEIGH SCATTERING VARIES WITH THE FOURTH POWER OF WAVELENGTH, SO RAYLEIGH(450 nm BLUE) > 4*RAYLEIGH(650 nm RED). A UNIQUE (FOR GIVEN ELEVATION) RAYLEIGH TERM THUS IS MEANINGFUL ONLY IN THE CONTEXT OF A SPECIFIC SPECTRUM. WHAT IS THE SPECTRUM IMPLICIT IN THIS DOCUMENT?]	The section has been revised.
Chapter 3, Section 3.2 (Figure 3-1) - [FIGURE 3-1 ACTUALLY DESCRIBES THE ESTIMATION OF NATURAL DECIVIEW LEVELS. THE TOP BOX SHOULD READ "Measurements of the Primary Components of $PM_{2.5}$".] Also Figure 3-1 caption needs correcting.	Both revisions have been made.

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Chapter 3, Section 3.3, first Parag. - Module C, with tandem [MY UNDERSTANDING IS THAT TANDEM FILTERS ARE USED AT ONLY A HANDFUL OF SITES] quartz filters, collects samples which are analyzed for carbon in eight temperature fractions and used to determine both organic carbon and light absorbing carbon concentrations.	Revision has been made.
Chapter 3, Section 3.3, Equation 1 - [WHERE DO THE COEFFICIENTS 0.944 AND 1.02 COME FROM? WHY ARE THEY NOT UNITY?]	This equation has been removed from the document.
Chapter 3, Section 3.3 - Light absorbing carbon (LAC) is also determined by TOR analysis and is calculated from the sum of carbon fractions oxidized at high temperatures minus the pyrolized fraction. [ELSEWHERE IN THIS DOCUMENT (e.g. THE GLOSSARY) YOU IDENTIFY EC WITH LAC.]	Revision has been made.
Chapter 3, Section 3.3 - Nitrate ion concentration is determined by ion chromatographic analysis of the sample collected in Module B. Assuming that the nitrate ion is associated with fully neutralized ammonium nitrate aerosol, [NH_4NO_3], the nitrate mass, [NITRATE], can be estimated from the nitrate ion mass concentration by using a multiplication factor of 1.29, which accounts for the mass ratio of NH_4NO_3 to NO_3^-. [KEEP IN MIND THAT SODIUM AND OTHER ANIONS CAN SHOW UP IN SOME LOCATIONS.]	This section has been revised.
Chapter 3, Section 3.3, Equation 3 - Fine soil mass concentration, [SOIL], is estimated by summing the mass of those elements predominantly associated with soil, with allowance for oxygen and other unmeasured elements [NOTE, FOR EXAMPLE, THAT 2.49 IS NOT SIMPLY THE RATIO OF SiO_2 TO Si.]	Revision has been made.
Chapter 3, Section 3.3, discussion of Equation 3 - In addition, a correction is applied for other compounds such as MgO, Na_2O, water [WHY ACCOUNT FOR WATER HERE AND NOT IN OTHER FRACTIONS, LIKE SULFATE AND NITRATE?], and carbonate [Malm, et. al., 1994a].	Revision has been made.
Chapter 3, Section 3.3, last Parag. - It is assumed that in rural areas of the country the primary constituent of coarse mass is [THAT'S A CAPRICIOUS ASSUMPTION!] suspended dust, along with some vegetative material.	This section has been revised.

Peer Review Comments - Technical Issues **Comment**	**Response**
Chapter 3, Section 3.4, Second Parag. - Most routine aerosol monitoring programs and many special study visibility characterization programs were designed to measure bulk aerosol species mass concentrations such as sulfates, nitrates, carbonaceous material, and selected elements [White and Roberts, 1977; [THIS PAPER INTRODUCED THE IDEAS YOU ARE TALKING ABOUT HERE, SPECIFICALLY THE ESTIMATION OF EXTINCTION BUDGETS FROM BULK AEROSOL MEASUREMENTS] Heisler et al., 1980; Malm et al. 1994b; Tombach and Thurston, 1994; Watson et al., 1990; Macias et al. 1981].	Reference added as suggested.
Chapter 3, Section 3.4, Second Parag. - For instance, Ouimette and Flagan [1982] have shown from basic theoretical considerations that if an aerosol is mixed externally, or if in an internally mixed aerosol the index of refraction is not a function of composition or size, and **[**the aerosol density is independent of volume**]** THIS QUALIFICATION IS HARD TO MAKE SENSE OF. YOU PROBABLY MEAN species densities are independent of composition.],	This section has been revised.
Chapter 3, Section 3.4, after Third Parag. - [THE PREVIOUS THREE PARAGRAPHS ARE OBSCURE AND CONVEY A CONFUSED UNDERSTANDING OF AEROSOL OPTICS. THERE IS NO MENTION OF PARTICLE SIZE AS **THE** MAIN DETERMINANT OF SCATTERING PER MASS. Knowledge of the particle size distribution all by itself, with no specific chemical information, offers a better estimate of the scattering coefficient than any amount of chemical information does. The apportionment of scattering to an individual species is theoretically justified only to the degree that species' distribution with respect to particle size is fixed, or at least independent of its bulk concentration (Vasconcelos et al., 2001).THERE IS REFERENCE TO "the specific mass scattering efficiency ... of the individual species" WITH NO ATTEMPT TO DEFINE IT INDEPENDENTLY (THAT IS, OUTSIDE OF EQUATION 5). In particular, there is no recognition that a_i is ill-defined for internal mixtures (White, 1986). If "the above studies demonstrate that a reasonable estimate of aerosol extinction can be achieved by assuming each species is externally mixed", as you observe, then it is equally true that many studies demonstrate that a reasonable estimate of aerosol extinction can be achieved simply by multiplying chemically undifferentiated fine mass concentrations by 3 m^2/g (e.g. White and Roberts, 1977), or perhaps 2 m^2/g in the arid southwest (e.g. White et al., 1994). The empirical success of the proposed "reconstruction" is weak evidence of its physical legitimacy.]	This section has been revised.

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Chapter 3, Section 3.4, start of 4th Parag. - In general the extinction contribution for each species is *modeled as* [AS NOTED ABOVE, THE CONTRIBUTION OF AN INTERNALLY-MIXED SPECIES IS ILL-DEFINED (WHITE, 1986)] the product of...	Revision has been made.
Chapter 3, Section 3.5, before the first Parag. - [THE FIVE PAGES OF DISCUSSION HERE ARE WAY DISPROPORTIONATE TO THE REST OF THE CHAPTER, ESPECIALLY GIVEN THE NEXT SECTION'S MUCH CRUDER REDUCTION OF F(RH) TO A MULTI-YEAR MONTHLY AVERAGE.]	No change made to document. Discussion provides appropriate level of detail.
Chapter 3, Section 3.6 (f(RH) values), before the first Parag. - [I THINK THIS IS THE WEAKEST LINK IN THE CHAIN, IN TERMS OF SCIENTIFIC RATIONALE. FOR THE DRY EFFICIENCIES YOU HAVE A VAST LITERATURE TO CITE, AND YOU APPROPRIATELY CLAIM ONLY ONE DIGIT SIGNIFICANCE. HERE, IN CONTRAST, THERE IS NO LITERATURE TO DRAW ON, AND YOU CLAIM THREE SIGNIFICANT DIGITS THAT ARE DIFFERENT EACH MONTH AND AT EACH LOCATION. AND ALL THIS WITH NO DISCUSSION OF SENSITIVITY TO CHOICES (e.g. 98% RH CAP) AND ASSUMPTIONS (e.g. CONCERNING DIURNAL CYCLES OF AEROSOL CONCENTRATIONS). I WOULD GO FOR SOMETHING MUCH SIMPLER AND MORE TRANSPARENT.]	The purpose of the f(RH) values is to capture effect of seasonal and regional humidity conditions on haze. We agree that the number of sig figs needs to be reduced and have done so.
Chapter 3, Section 3.6, third Parag. - for each mandatory Federal Class I area..[8] [WHAT DOES THE SUPERSCRIPT "8" REFER TO?]	It refers to the footnote at the bottom of the page; no change made.
Chapter 3, Section 3.6, third Parag. - The hourly RH measurements (equal to or below 98% RH [THIS IS AN AWFULLY HIGH VALUE TO MEASURE WITH THE ACCURACY DEMANDED BY THE SENSITIVITY OF F(RH) IN THIS REGION. HOW MUCH WOULD YOUR MONTHLY AVERAGES FOR F(RH) DROP IF YOU RESTRICTED YOUR CONSIDERATION TO 90% RH?]) from each site were converted to *f(RH)* values using a non-linear weighting factor curve (see Figure 3-2).	This section has been revised.
Chapter 3, Section 3.6, last Parag. - The results of that work are the values presented in Appendix A. [SEE COMMENTS IN APPENDIX A.]	No change made in Section 3.6.

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Chapter 3, Section 3.8, first Parag. - A **standard value of 10 Mm^{-1}** for Rayleigh scattering is used in visibility calculations regardless of site elevation to simplify comparisons of values among sites at a variety of elevations. [THIS MAY "SIMPLIFY COMPARISONS" FOR EXTINCTION, BUT IT DOES SO BY DISTORTING DECIVIEW COMPARISONS, AND DECIVIEWS ARE YOUR BOTTOM LINE. CONSIDER THE EXAMPLE OF A SEA-LEVEL SITE, WITH 13 Mm^{-1} RAYLEIGH, 5 Mm^{-1} NATURAL BACKGROUND AEROSOL, AND 10 Mm^{-1} ANTHROPOGENIC HAZE. YOU FIGURE THE NEEDED IMPROVEMENT TO BE 10LN(25/10) - 10LN(15/10) = 5.1 DECIVIEWS, WHEREAS BY YOUR LOGIC IT SHOULD BE 10LN(28/10) - 10LN(18/10) = 4.4 DECIVIEWS. THIS EFFECT IS ADMITTEDLY NOT HUGE, BUT IT IS LARGER THAN OTHER THINGS YOU ARE ACCOUNTING FOR, SUCH AS THE EFFECT OF HUMIDITY ON BACKGROUND EXTINCTION LEVELS. (OF COURSE SEASONAL TEMPERATURE SWINGS ALSO AFFECT RAYLEIGH.)]	The sentence has been revised.
Chapter 3, Section 3.8, second Parag. - Finally, William Malm (**personal communication** [OK, LET'S SEE IT – WHAT ARE APPENDICES FOR?]) has compared...	Reference added to this discussion.
Chapter 3, Section 3.8, second Parag. - ...and found that on average there was only a 1.2% difference between the two values. [YEAH, AND "ON AVERAGE", EVEN-NUMBERED DATES AND ODD-NUMBERED DATES PROBABLY EXPERIENCED SIMILAR VISIBILITIES. THIS IS NOT A VERY MEANINGFUL STATISTIC.]	The sentence has been revised.
Chapter 3, Section 3.9 - ...which **is a small but perceptible scenic change** [THIS CLAIM, WHICH IS NOT UNIVERSALLY ACCEPTED, IS CLEARLY UNFOUNDED IN THE PRESENT CONTEXT, WHERE THE OPTICAL INDEX UNDER CONSIDERATION IS BASED ON 24H AVERAGE AEROSOL MEASUREMENTS AND 10Y AVERAGE RH DATA. WHAT DOES "PERCEPTIBILITY" MEAN AT THESE AVERAGING SCALES?] under a wide range of visibility conditions.	The sentence in question is only helping to define the haze index with deciview scale, not trying to layout what is perceptible with regard to the averages used in the haze rule. No change
Appendix A, page 6-4 - The U.S. National Park Service has tested this possibility, by examining data for each of the 12 months from 20 mandatory Federal Class I areas where relative humidity measurements are made. In **nearly all cases**, no statistically significant correlations were found between measured concentrations of SO_4^{2-}, NO_3^- and [SO_4^{2-} + NO_3^-] vs. daily values of relative humidity **in a large majority of months**. [GIVEN THAT THIS IS UNPUBLISHED WORK THAT CONFLICTS WITH PUBLISHED WORK (e.g. VASCONCELOS ET AL., 1994), THIS CLAIM REQUIRES BETTER DOCUMENTATION.]	Appendix B provides further elaboration on the analysis of these correlations.

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Appendix A, last two sentences of text - These results **suggest** there is a **relatively** weak correlation between hygroscopic components of PM and relative humidity and that the choice of a "climatological" vs. "day-specific" method for computing *f(RH)* has little apparent effect on observed trends in visibility. [DITTO ABOVE COMMENT] Consequently, the simpler climatological approach is used in regional haze calculations.	Appendix B provides further elaboration on the analysis of these correlations.
Comments of Bruce Hill (via the Clean Air Task Force):	
Comments of Bruce Hill on the Natural Conditions Document	
I. It is critical that EPA provide states with a stable methodology for determining baseline natural conditions because they will represent the yardstick against which decrements of anthropogenic impairment under the regional haze program will be measured. Precision–that is, the repeatability of results–requires that the procedure chosen must be a well-defined and stable over time. For this reason, we believe that states should always present estimates using the recommended EPA default methods, for comparison, even if they intend to use other techniques, such as refined estimates of natural background, in their long term planning and progress SIPs.	It may be important to compare default and alternate approaches only during the initial SIP. This issue would be addressed during the review of the SIP.
A. To ensure that real progress is achieved in meeting the progress targets established in states' visibility SIPs, a consistent method should be utilized by states for estimating natural conditions from the outset of the regional haze program until the national goal is met, in order to ensure precision. Changing methods midway through the process could result in "apples to oranges" estimates of progress, and would encourage gaming of the long-term strategy. Thus we believe that it is critical that the original estimate of natural conditions be utilized in tracking progress throughout the program, whether it is derived from the default method or a state-specific "refined" method as provided for in section 3.4 of the guidance. In addition, any estimate of natural conditions using a refined method should be accompanied in all cases by an estimate of natural conditions using the default method. This will serve as a benchmark for any refined estimates, and allow EPA to compare programs in neighboring states and airsheds.	Agreed. If the rules change they must be applied consistently for all years.

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Peer Review Comments - Technical Issues **Comment**	**Response**
B. We support the use of the NAPAP values as a starting point for the process in the first planning period. Appendix a of NAPAP Report 24 includes the most comprehensive analysis of natural conditions undertaken to date. It required several years of research and would be difficult to update in time for the first planning period. It does have one limitation, which is that it represents the application of only 2 sets of background values to fit the entire United States. Although modification by regional or local f(RH) values will lend more accuracy to the estimates, some regional break-outs would be desirable, particularly between areas that may have different causes of natural haze (e.g., wildfire, dust, other organics) or concentrations of naturally occurring PM such as between the northern and southern Appalachians. We believe refinements should not be simply left up to the states. Instead, EPA should take a lead role in undertaking refinement of estimates applying a single method nationally–if possible, in time for planning SIPs.	This is a policy decision for EPA(i.e. to fund more research on natural haze levels), and is not addressed in this document.
C. We support adopting the IMPROVE methodology, in conjunction with methods to calculate monthly average f(RH) values, to arrive at a stable long term standardized quarterly relative humidity factor f(RH), as provided for in the guidance.	No change needed.
D. We agree with EPA that internal consistency in methodologies is critical in order to ensure that real progress is made towards meeting established targets.	No change needed.
E. We request clarification from EPA on why quantifying "infrequent natural impacts" or outliers is desirable. It is unclear why stochastic events would not already be accounted for in the more stable and deterministic long-term average values.	The document does not say states should do it that way, only that they can. It would be up to the states to indicate that the refinement approach is advantageous. No change made to document.
F. In section 1.10 of the guidance, EPA states that estimates of natural visibility conditions should reflect contemporary conditions and land use patterns and vegetative landscapes for practical reasons. From the brief discussion it is unclear how this approach might affect states in their efforts to meet the Clean Air Act's national visibility goal. We request EPA to address this issue in more detail.	The natural visibility goal will represent conditions in 2064.
Comments of Bruce Hill on the Tracking Progress Document	

Peer Review Comments - Technical Issues **Comment**	**Response**
II. In general, we support EPA's recommendations that internally consistent metrics for estimating progress based on the IMPROVE program should be used consistently throughout the implementation of the regional haze program, in order to ensure internal precision in estimates of progress. The tracking index is, perhaps, the most important technical component of the Regional Haze Rule, as it will ensure the goals of the regional haze programs and the national visibility goal are met. It is critical that a stable methodology be provided in the guidance to ensure a systematic, consistent measure of progress so that tracking methods do not change with time.	No change needed.
A. States Without Class I Areas Within Their Borders, But Which Impact Class I Areas in Other States, Should be Required to Incorporate the Progress Goals for Those Areas in Their SIPs. Under the Regional Haze Rule, a state with no Class I areas within its boundaries is not required to establish any reasonable progress goals in its SIPs, but is required to consult with states having Class I areas that might be impacted by its emissions. We believe that the Tracking Progress guidance would be greatly improved by requiring the states which do not include Class I areas, but which do include sources that are reasonably anticipated to cause or contribute to visibility impairment in another state, to at least confer with other states with impaired class I areas in the affected RPO or region, and incorporate those progress goals in their initial and later SIPs. This requirement would be consistent with the Regional Haze Rule. *See* 40 C.F.R. § 308(d)(3). It will be particularly helpful in enabling careful planning for states that are outside of the Regional Planning Organizations ("RPOs") which already have been established. We envision that a state, with or without Class I area within its boundaries, may ultimately need to participate in more than one RPO. For example, Midwest states may cause or contribute to visibility impairment in the MANE-VU (Mid-Atlantic and Northeast RPO) region, VISTAS (Southeast RPO), and/or the Central States Air Partnership ("CENRAP") RPO region. For this reason, a state should be engaged with all RPOs that are impacted by emissions from that state. And states will need to be fully aware of and should document progress targets in all states that may be affected by emissions from all upwind states. EPA should address the problem of states affecting more than one RPO more comprehensively in revised guidance.	Not the topic of these guidance documents, but RPO's are communicating on cross-RPO impacts.

Peer Review Comments - Technical Issues **Comment**	**Response**
B. The Slope of Improvements as Measured in Deciview Should Always Be Equal to or Greater than the Value of the Linear Slope Between 2000-2004 Conditions and Natural Conditions. The method proposed by EPA for tracking progress correctly requires the estimate of a linear slope or path between the baseline conditions and natural conditions over the implementation period for the program. It is possible that some states will wish to propose a less aggressive approach in the early planning periods and put off meaningful progress until a later date when they would catch up. Conversely some states may wisely start with more aggressive goals. It is important that the slope of the improvements in any five-year planning period should be greater than the slope of the linear "glide path" between the 2000-2004 baseline conditions and natural conditions, divided by the number of years. And of course the rate of improvement under the Haze Rule must always be greater than the improvement achieved through all other Clean Air Act programs.	No change needed.
C. Emissions Reductions Goals Should Be Established and Tracked EPA's guidance relative to tracking emissions reductions programs is poorly defined in this document. It is simply noted in section 1.11 that progress in emissions reductions programs are an integral part of the overall criteria for measuring a state's progress toward meeting the targets established. We believe that a supplemental recommendation that emissions baselines and progress goals should be similarly established and tracked under the regional haze SIPs in parallel to visibility conditions and goals. Under this method, states would model the emissions reductions needed to achieve natural conditions up-front and stipulate progress goals based on the needed reductions. These progress goals would be tracked between each planning period and should amount to no less than the linear rate of progress needed to achieve natural conditions in downwind Class I airsheds by 2064. We request that EPA consider adopting this recommendation and provide comment on this concept in its response.	The topic of emissions reductions tracking is not the subject of these guidance documents. No change needed.
D. Including Data from Incomplete Years Should be Allowed to Bring the Five-Year Average Closer to Its True Value. We agree with EPA that "... it is reasonable to include the highest 20% deciview readings from an incomplete year if those values increase the five year average of the highest 20% of deciviews relative to that based on complete years only." Tracking Progress Guidance, § 2.3 at 2-10. Indeed, we believe that this directive should be strengthened, so that the language would instead read: "Thus states *should include* the highest 20% deciview readings from an incomplete year if those values increase the five year average of the highest 20% of deciviews relative to that based on complete years only."	The requested intent is already established by the current wording, no change.

Peer Review Comments - Technical Issues **Comment**	**Response**
E. Outlier Data Should be Included in Annual Estimates of Worst and Best Days. We agree with EPA that outliers should be included for internal consistency and precision in calculating the 5-year average condition, as outliers have already been incorporated in the calculation of natural background. Tracking Progress Guidance, § 3.10 at 3-16-3.17. We further agree with EPA that if an outlier is clearly not representative of regional haze conditions (e.g., it represents a structure fire), it should be flagged and explained adequately. However, in many instances an outlier day may be the confluence of a regional haze event and an unrelated local event and if at all possible, the regional haze data should not be discarded if at all possible. For example, on the Fourth of July, fireworks in proximity to monitoring sites may result in the recording of abnormally high PM2.5 levels, yet the underlying regional haze event may be among the 80th percentile days and thereby important not to discard. Explanations of such events should be rigorous and document the nature of the excursion including timing of events, location of monitor relative to PM source, and perhaps identify anomalous chemical signatures in the PM mixtures if possible. A sample should not simply be discarded and flagged but should instead be reevaluated using any available methods to reconstruct the underlying regional haze mass. For example, outlier data should be examined in light of data from other nearby but otherwise unaffected monitoring sites if such data is available. Finally, EPA should recommend to states that monitors that systematically record significant excursions or outliers should be moved to new locations.	These issues of monitoring representativeness are addressed through the states and the IMPROVE network. The guid docs are based on the assumption that sites are regionally representative.
F. If EPA Creates New Methodologies to Calculate 5-Year Deciview Averages, in Situations Where a Five-Year Period has Fewer Than Three Complete Years of Data, That Would Constitute a Significant Substantive Change in the Guidance Which Should be Subject to Further Public Scrutiny. In section 4.3 of the guidance, EPA indicates that if a five-year period has less than the stipulated 3 minimum years of data that the estimate should be prepared through consultation with EPA/OAQPS. EPA leaves open the possibility that it would develop new methodologies for calculating 5-year deciview averages for the purpose of estimating reasonable progress where fewer than 5 years of data are available. We are concerned that the development of such new methods would constitute s significant revision to the guidance that would require additional public comment.	All revisions must be done through a SIP process and review by EPA and the public occurs at that stage.

Peer Review Comments - Technical Issues **Comment**	**Response**
G. A Minimum of One Year of Co-located Sampling Should be Required when Significant Procedural Changes are Made in IMPROVE Sampling Protocols. When procedural changes in IMPROVE occur in the future, co-located comparative sampling should be required for a period of no less than an entire year. This is important to ensure that estimates of progress are not confounded by sampling bias. Data generated using the preexisting method should be compared to the revised method using linear regression and other statistical measures of similarity between the two methods to determine if corrections must be made in initial baseline conditions, natural conditions estimates and measures of 5-year progress to match the new methodology's estimates in future planning periods.	This should be considered by IMPROVE and doesn't need to be addressed in the guidance documents. No change made.
H. Progress in Reducing All Particulate Matter Species Should be Tracked Individually. EPA states in section 4.7 of the guidance that trends in the individual particulate matter species are important, and cites, as an example, the importance of tracking sulfate in areas where sulfate is the dominant cause of visibility impairment. We believe that all species should be tracked, not just the dominant ones. As dominant species are reduced relative to all species, as a result of the regional haze and particulate matter control programs, other species may become increasingly more important, both in a relative sense and in terms of emissions growth, or changes in atmospheric chemistry. For example, in ammonia limited areas, as sulfur dioxide gas is reduced, nitrogen oxides present will combine with the liberated ammonia to form ammonium nitrate instead of ammonium sulfate. In this situation, nitrate aerosols could conceivably replace sulfates with little net improvement in visibility and related progress. For this reason, tracking *all* species will be important.	The text has been revised.